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Firstly, I thank the reviewers, all of whom have raised important issues of ethnographic interpretation. In this reply, I engage their broadest themes first with a comment on the discursive framing of *What Gifts Engender (WGE)* and then with responses to the more specific queries posed. Especially in the second half of this response, I address another, more theoretical matter, to which the reviewers variously point: that is, the need to rethink existing modes of cultural comparison.

Dialectics of the Gift

As Mosko notes, *WGE* was offered partly as a critique of ecological and production-centered analyses of Highlands societies. But while the critique is explicit in the book, it was not meant to stand on its own. Rather, it was embedded in a more complex project, the aim of which was to weave together two representations of Mendi history: one a structural analysis of gendered sociopolitical relations bearing on what ecological anthropologists working in the Highlands have called "pig cycles," and the other an account of the events and contingencies that my informants deemed relevant to the staging of the 1979 Suolol pig festival (*mok ink*).

However, as Mosko also suggests, unlike other recent critiques, *WGE* did not seek simply to substitute exchange for production; in fact, it aimed to collapse the production/exchange distinction. In the language I used in the book, exchange is itself "a relation of production"--a construction that I meant to be compatible with, though not strictly derivative of, neo-Ricardian notions like those of Gregory (1982) or Gudeman (1978), and structural-Marxist ideas about kinship like those of Godelier (1977; see particularly Merrill 1976, 1979/1980). Insofar as I was dealing with embodied forms of sociality, I could have also turned the formula around: social relations of production could have been considered as exchange relations. But I preferred the first phrasing's critical edge.

As contrastive emphases intimately related to the analysis of our own economic institutions, the "exchange" and "production" perspectives

have had intertwined intellectual histories in Western scholarship both within and outside anthropology. I believed that the convergence of structural and Marxian anthropologies cuts across this opposition in a potentially liberating manner, while retaining an emphasis on power relations ("property," "exploitation"). However, I also wanted to avoid sociological reductionism and a universalized concept of practices and of self-evident interests or ends (cf. Bourdieu 1978).

Thus, I sought to pay attention to those historically received relations (cultural "structures") that shaped the conventional, or given, meanings of people's actions in Mendi, as well as to the conservative or subversive plays on/for those meanings ("politics") that people made in the course of events I studied. I emphasized the mediated social relations embodying or acting out what we might represent as local ideas about social "agency" (effectiveness, productivity) as *analogues* to the Marxian concepts of "property" relations and "production" ("labor") in capitalist societies. This focus turned out to be fortunate since, in Mendi, prestations of all sorts are subject both to practical and to explicit discursive elaboration. Attention to them was then a condition for unpacking received categories of comparative analysis (like "property") and reversing the direction of translation so that indigenous notions could be presented with some integrity and subtlety. It also opened the way for commentary on Mendi perspectives on market-oriented development (chap. 7).

The immediate and most important pretext for my work was Roy Rappaport's celebrated study of the Maring *kaiko*, *Pigs for the Ancestors* (1969). I originally chose to study Mendi pig festivals (*mok ink*, or *sai le*) thinking that they might provide an interesting comparative foil to the *kaiko*. Insofar as both Mendi and Maring staged periodic "pig kills," they appeared structurally similar to one another; both differed from central Highlands exchange systems--like the Mae Enga tee or the Hagen *moka*--which involved enchained prestations of live pigs between groups. But Mendi production was more similar to the intensive gardening and pig husbandry practices of Mae and Hagen than it was to that of fringe Highlanders like the Maring. Thus Rappaport's ecologically conservative interpretation of Maring pig cycles was as inapplicable to the Mendi as it was to those other central Highlands peoples whose *longues durees* had involved significant ecological transformations (see, e.g., Golson 1982; Lederman 1986a).

In this way, Mendi appeared to be a wonderful vantage for reassessing comparativist arguments concerning the structural entailments of different local production/exchange regimes: for example, Rappaport's

on the determinants of the periodicity of pig kills, or A. Strathern's concerning "finance" and "home production" as alternative methods of acquiring valuables for prestations (1969, 1978, 1985). This literature concerned fringe/central Highlands differences but had a decidedly northern bias, a point three of the reviewers note and to which I will return.

However, the point of my project was not simply critical. By the time I set off for Mendi in 1977, Rappaport's study had already been subjected to scrutiny (e.g., Brookfield 1972; Hide 1981). It appeared to be time for nuanced interpretations that engaged Rappaport's arguments and also took into account the criticisms he had himself leveled at a previous style of political anthropology. Thus, for example, I agreed that altogether too much emphasis had been placed on male leadership styles in the study of large-scale, clan prestations. In order even to understand collective events themselves, it seemed necessary to go off-stage (although I did so in a different way from Rappaport): to report something of everyday prestations and of the knowledge, values, and concerns of women and ordinary men. At the same time, framing the study of local politics in terms of the structure of social relations made it possible to study cultural meanings without reducing matters simply to the intentional or individual.

Again, the Mendi proved appropriate in several ways. Inviting comparison with northern central Highlanders like the Mae and Hageners--about whom there was already an extensive literature--the Mendi were also similar to other southern Highlanders for foregrounding noncollective relations of various sorts. While I would not call the Mendi (or other Southern Highlands Province social orders) "loosely structured," they are certainly decentralized politically relative to their northern neighbors, and their big-men tend not to be that big (Lederman 1990a, 1991). Consequently, I was not as ineluctably drawn into research on big-men and groups as other central Highlands ethnographers had been.'

It is also relevant that--being married to a historian (who came to PNG with me together with a trunkful of relevant reading materials), being fascinated by the literature on Highlands prehistory, and working my way to what we might now call a critical poststructuralism--I framed my research questions as a kind of local history, aiming to pay special attention to the dialectics of social life (Murphy 1971): to conventional forms of creativity and to subversions of convention, to structural contradiction, and to cultural anomaly whether of persons, objects, or events.

The aforementioned suspicions concerning formal, male performance, together with these sorts of theoretical leanings predisposed my attention to gender relations. Both the critiques of Rappaport's work and positive ethnographic studies (e.g., Friedman 1974; M. Strathern 1972) suggested that conflicts between men and women (and perhaps among prominent and ordinary men) over the intensity and ends of garden work might limit the scale and frequency of male-dominated ceremonial exchanges. Mendi was particularly interesting in this regard, since Ryan's earlier work suggested that Mendi women controlled the distribution of their own bride-wealth prestations to a degree. Did Mendi women's agency extend beyond this, and did it account for the relatively long period between Mendi pig festivals?

Once in Mendi, I came to realize that the kind of analysis that treated "men" and "women" unproblematically as opposed groups or classes was inadequate, the literature on Highlands "sex antagonism" notwithstanding (as Strathern also points out in his review). Attention to the form taken by diverse exchange relationships--and particularly, as Mosko, Rumsey, and Strathern note, to noncollective, differentiating ones (e.g., personal exchange partnerships; *twem*)--made possible an alternative interpretation based instead on an indigenous categorical distinction between clan (*sem*; "family" or "kind") and *twem* relationships. Analytically, this distinction was only implicit in Ryan's work; I made its general importance in structuring Mendi experience explicit.

These two forms of sociality, and their mutual relations, are "gendered." Whereas clanship is a collective and unitary (exclusively male) relation, exchange partnerships are based on situationally defined asymmetries (e.g., affinity, or a differential need for valuables). In contexts in which men seek to persuade one another about the value of collective (male) projects, network relationships are rhetorically associated with "femaleness," reflecting divergent and particularized interests--even though both men and women participate in them and men statistically more so. However, clan and network cannot be tagged simply as "male" and "female." Local gender meanings are used relativistically; and they have an overtly political, not objective, value.

While the association of maleness with collectivity and femaleness with its opposite may appear familiar, in fact Mendi gender roles and values do not map neatly onto ours. For example, women's network relationships link them with people outside of their husbands' and their own natal communities: their concerns can be considered neither simply domestic, local, nor "internal." The marital relationship has something of the feel of an exchange partnership (rather than a corporate

"unit"); in any case, women are never entirely alienated from their natal communities, in which they retain gardens and to which they return when things are not going well for them in their husband's place.

And (as Rumsey's and Strathern's reviews point out), neither is the distinction between clanship and personal exchange network equivalent to that between the abstract and totalizing Western notion of "society" and its antithesis, the autonomous, self-interested "individual" (see also Lederman 1989). "Autonomy" (in the sense of attributions of specifically personal responsibility or agency in events) is locally understood to be a function of social relationships. What is more, neither clanship nor *twem* partnership is represented unproblematically as subsuming the other; because each is represented as both a source and a product of the other, their mutual relation cannot be considered hierarchical. While in practice their demands may conflict, the conventions and rhetoric of exchange also work to conjoin them so that they can even be construed as transformations of one another.

As it turned out, reorienting my structural focus to the network/clan relationship helped me to answer the original questions in which I had been interested concerning the systemic constraints on pig festivals and the particular political dynamics of community events. It became clear that Mendi pig festivals were not cyclical, not ritual inevitabilities but contingent political achievements. I found I could also account for the short-term structure of production--which resulted, as Mosko notes, in the rapid buildup of pig herds after pig festivals, and their maintenance at relatively constant, high densities even in the absence of plans for local collective events. This pattern turned out to be similar to that described for Simbu (Hide 1981), but quite different from Maring. Longer-term patterns--involving elements both local and (shall we say) exotic--were also intelligible precipitates of the structural dynamic I have outlined.

Comparative Strategies

I have described *WGE*'s themes in some detail as a framework for addressing those of its reviewers. Firstly, I appreciate Mosko's explication of the book's contribution to debates about "production" or "exchange" models; I hope that my remarks help to qualify the sense in which the book adopts an exchange (or "distribution") approach. Similarly, Mosko, Rumsey, and Strathern make complementary points about *WGE*'s ethnographic contribution to a "post-Durkheimian" anthropology.

As I have noted above, I organized *WGE* around ethnographic issues derived from northern fringe and central Highlands ethnography. Mosko, Rumsey, and Strathern all comment on this strategy. Now, a north/south comparative axis was itself a kind of innovation, given the conventional east/west polarity of Highlands-centered comparisons (e.g., Watson 1964; Feil 1987). However, given this reorientation, Mosko rightly asks about the appropriateness of my focus on the Mendi's northern neighbors, and deemphasis of southerly comparisons.

There is no doubt that *WGE* would have benefited had it drawn more explicitly on the ethnography of the southern Highlands fringe (the longhouse peoples like the Etoro and Kaluli) or of southern Highlands grasslanders (like the Wola, Huh, and Duna). While *WGE* does refer to these peoples, and while I framed Mendi ethnography with the emerging southern Highlands literature in mind (e.g., pp. 20-21, 62-65), specific ethnographic connections were left largely implicit. Recently, I have begun to develop these southern connections directly (Lederman 1987, 1989, 1990a, 1990b, n.d.).²

Mosko's pointed queries raise other, more general questions about strategies of comparison. Why should the Mendi be compared with other southern Highlanders rather than with the Mae Enga, Melpa, Maring, or Simbu? The formal, structural criterion ("family resemblance") that Mosko introduces needs to be unpacked: Are some kinds of resemblances more important than others for (some? all?) comparative purposes? I doubt that one could argue that the Mendi's affinity with the Etoro, Kaluli, or even Huh is "greater" than that with the Enga (to whom they are linguistically related, and from whose heartland they claim to have emigrated) or the Melpa (whose exchange categories parallel theirs, as Rumsey notes). One way or another, the Mendi resemble (and also, obviously, differ from) each of these folks.

Any discussion of comparativist strategies must begin by recognizing that cultural comparison involves not the discovery of natural or objective facts, but the invention of categories. Where one draws the cultural lines is an artifice. However, it is not arbitrary; it rests on assumptions deriving from specific discursive contexts.

While there are other contextual constraints too, perhaps the most important is disciplinary. Like most ethnographies, *WGE* reflects its author's engagement with anthropological conversations at least as much as with local, Melanesian ones (Lederman 1990c). I emphasized the northern central Highlands literature because I recognized that Mendi was already implicated in that conversation (as Rumsey also notes in his discussion of confusions concerning the apparent similarity

between the terms "*moka*" and "*mok ink*"; see, e.g., Feil 1987; Rubel and Rosman 1978; and references in both A. and M. Strathern's publications). I wanted to help reorient its direction.

This rationale explains my emphasis on the Stratherns' Melpa (Hagen) ethnography. Addressing Mosko's question about that emphasis, it did not signify a belief that the Melpa case "epitomizes" central Highlands cultures. The point was that their writings have had a central role in shaping central Highlands anthropology. That is, the Stratherns have, on the basis of their research in the Mt. Hagen area, put a number of issues on the table that my experience in Mendi enabled me to address, introducing a novel, "southern" perspective. In this regard, I thank A. Strathern for the extremely useful ethnographic clarifications in his review, particularly concerning the significance of conflicting allegiances among Melpa men. His comments dovetail with Rumsey's and suggest the possibility of a "southern" reading of key "northern" cases: just the sort of rethinking--in this case, shift in interpretive emphasis--I had hoped the north/south reorientation might provoke.³

Now, there are at least two ways of using ethnographic cases like this to engage comparative arguments. The first, arguably dominant, way is governed by comparative anthropology's positivist logic. Thus, insofar as our concern is the construction of sociological types, anthropologists search for similarities (using formal, structural criteria alone or in combination with geographical ones), hoping to "discover" lawlike regularities in the associations of sociocultural variables.

This style of comparison engenders a host of knotty problems that, when they are considered at all, are often treated only as technical challenges, not as matters calling the basic method into question (but see Wagner 1981; also Modjeska 1982, whose self-critique provides an insight into some of the problems referred to here). For example, under what circumstances is it in fact justifiable to emphasize the cultural similarities of geographically contiguous peoples (e.g., "southern Highlanders")? To the extent that comparison is based on formal similarities, how are we to justify our selection of features (alternatively, how do we rationalize the choice of contextualization by means of which our selections appear meaningful and nonarbitrary)? Very generally, in what senses are social orders "systems"; what kinds of constraints on transformations should our constructions presume?

For example, I suggested that a key contrast between southern and northern central Highlanders (between, say, the Mae and Hageners on the one hand and the Huli, Wola, and Mendi on the other) appears to be the relative values of differentiating (e.g., network) and collectivizing

(e.g., clan) relations. Rumsey and Strathern explore some implications of this difference for gender meanings, in particular a possible correlation between the elaboration of exchange networks and female power. Strathern even questions the validity of Sillitoe's male-centered analysis of Wola exchange on this basis.

But tempting as such a correlation is, doubts about "positive" styles of comparison force me to question it. The problem is that exchange networks do not have a necessary or inherent value that may be extrapolated in a linear fashion from one ethnographic case to the next. The significance of networks is relative, and their "positional value" (to use Sahlins's phrase) is subject to alteration with changes in that set.⁴ Neither are southern (or northern) Highlanders necessarily all of one sort. In fact, neighbors sometimes organize themselves as if in counterpoint to (or else as mistranslations of) one another's practices. In a word, while I am far from abandoning a notion of social "systems," our notion of systemic relationships needs to become less functional and abstract, more dialectical and sensitive to the ways in which our informants mediate their own historically experienced differences.

Consequently, in common with several others (notably Kelly), my own comparative strategy in *WGE* and subsequently has been primarily deconstructive. Rather than proposing covariations of my own, my aim has been to report instances that break existing ethnographical patterns, that foil typological generalizations, that undercut functional arguments. That is, despite its many similarities with other central Highlands cases, I found that Mendi also differed in significant ways that could not be accounted for neatly within the terms of existing comparative syntheses. For this reason, I have treated the Mendi as "southern" to mark them off as "not northern"; that is, as potentially subversive of ethnographic categories (e.g., "big-men") which we have construed as Highlands-wide. In this way (and as Rumsey's comments suggest), I have sought to make space in Highlands ethnographic debates for some of the issues with which southern Highlands researchers have been concerned (see especially Weiner 1987).

Although constructing the category "southern" has a certain critical value, I thoroughly expect it to break down into other sorts of relativities. Working within the newer southern Highlands ethnography does not require that one develop regionally focused comparisons in a positive mode: Mendi ethnography can be used in the same argumentative manner in this context as well.

The point of this strategy--quite explicit in *WGE* and elsewhere (Lederman 1986a, 1989, 1990a, 1991)--has been to explore the limita-

tions of certain social-science styles of writing and to assert, if not adequately to show, the value of those humanistic styles that seek to convey the "open destinies of life" even amidst an accounting of its historically constituted constraints. This goal has motivated a rethinking of comparative analysis about which I am more explicit in my forthcoming work.

Alternative approaches to comparison may draw upon anthropological history (Sahlins 1981, 1985) and those aspects of feminism and post-modernism that center attention on contradiction and the generative conditions of culturally particular styles of innovation. As I have suggested above, local discourses on difference may be brought productively to bear on anthropological constructs. Thus, I have been interested in how the Mendi understand their relationships with the peoples with whom they interact (including Westerners) and the manner in which "external" relations are modeled on "internal" relations of difference and sameness (e.g., played out in terms of gender and exchange). From this perspective, the emphasis is on difference as much as similarity and the point is not so much to map functional regularities as it is to understand transformations of various sorts.

In light of these concerns with history, comparison, and ethnographic writing, Ryan's review requires a special response. He is concerned about the discrepancies between our two accounts of Mendi society, and asks to what extent they can be attributed to postcolonial changes--to "a shift in emphasis . . . of the whole system"--and to what extent to our different styles of ethnographic interpretation. These are important questions, but narrow misreading trivializes Ryan's contribution; I must confess that I am disappointed (although unfortunately not surprised) by his tone. As Rumsey and others have noted in reviews of *WGE*, I have consistently treated Ryan's work with care and respect; I suppose I expected a degree of reciprocity.

Firstly, Ryan questions my analysis of Mendi women's engagement in *twem* exchanges, wondering if perhaps this is a function of recent transformations in Mendi society or changes in anthropological interpretive fashion (on these points, see Lederman 1986b, 1989). However, it must be said that the credibility of Ryan's assertions about women's lack of participation in the 1950s is undercut by his admission that he "for various circumstantial reasons, did not, at that time, have access" to the "female side of Mendi life." He commented similarly in his unpublished doctoral thesis on Mendi (Ryan 1961:83). A full accounting of the "various . . . reasons" might have helped us to disentangle error, incompleteness, and theoretical bias from the historical changes in which we are both so interested.

It must also be said that Ryan's comment concerning the predominantly intergroup character of *mok ink* during the 1950s is belied by statements he himself made in his thesis: for example, that the "real importance" of *mok ink* "lies in the vast network of minor exchanges that have been taking place between the hosts and their *twem* partners" (Ryan 1961:219). For my part, I never reduced *mok ink* to its noncollective, network aspect; on the contrary, the main point of chapter 6 on the Suolol pig festival (see, e.g., p. 212) was to show how the festival integrated and ceremonialized *both* clan and network interests. (I thank Mosko, Rumsey, and Strathern for developing related points in their reviews.)

All in all, while it is obviously untrue that I ignored the "historical dimension" in *WGE*, it certainly *is* the case that I did not--could not--use Ryan's work--whether concerning the relative positions of agnates and nonagnates, women and men, or (military) alliances between clans and exchange networks--as an unproblematic baseline concerning "Mendi in the 1950s" from which to speculate concerning postcolonial historical change. He is understandably peeved by this. He apparently considers his findings to be transparent, and implies that the corporate-group emphasis of fifties anthropology (not to mention its male bias and its functionalism) had little to do with his own emphases. However--a paragon of the old double standard--he puts great stress on the perverse influence of my newfangled (feminist) "theoretical interpretation."

Of course, as any undergraduate knows, it is inevitable that "theory" shaped what both of us saw (the almost total lack of literature citations in Ryan's thesis notwithstanding). The point is that both Mendi ways-of-seeing and our own are historically particular; representations of cultural change that seek to interpolate reports by anthropologists working in the "same" place at different times need to bear both kinds of history in mind.⁵ While overall I was impressed with how well our two accounts meshed, there were several notable instances in which I felt fairly sure that interpretive bias explained our differences. An example is Ryan's analysis of the circulation of wealth in *twem* networks (in which, among other things, he imposed an ethnocentric concept of "ownership" that also limited his understanding of women's participation in exchange; see *WGE*, chaps. 3, 4). Most generally, I took seriously his many cautions concerning the severe constraints (both circumstantial and self-imposed) under which he had conducted field research (e.g., Ryan 1961:i-iii, 73, 83), and I limited my use of his work accordingly.

I will not comment on Ryan's disquisition about social structure except to say that it is embarrassingly naive with respect to the

published literature and a complete non sequitur to the book under review.⁶ Ryan's "boring non-issue" is apparently "tenacious" for him alone: to my knowledge, no one has ever contested the vague point that peoples like the Mendi are "patrioriented"!

A final comment on matters undeveloped in the book under review. Strathern reiterates Ryan's concerns when he asks whether *WGE*'s sequel will be "What Money Engenders." I agree completely with Strathern's sense of the future needs of New Guinea ethnography. Indeed, my forthcoming book (Lederman n.d.) is very much about the contradictory and reflexive processes of cultural innovation in postcolonial Papua New Guinea, paying particular attention to the (analytically problematical) gift/commodity distinction. However, I would qualify the definition of "historical change" implicit in the last section of Strathern's review (and probably also in Ryan's remarks as well). As my opening paragraphs here suggest, in *WGE* and elsewhere (see especially Lederman 1986a) I do not define "history" narrowly as the documented past nor as postcontact change, but work with a broader definition that includes precontact social process as well as indigenous constructions of various sorts (e.g., of what we might call "agency" and "transformation"). Thus, while it is true that I dealt in detail with postcolonial change only in chapter 7 of *WGE*, that is by no means the only "history" in the book. The analysis of structural contradiction (chaps. 2-5) was a necessary component of any historical (processual, dynamic) understanding of Mendi sociality during the precontact and postcontact periods, and the analysis of the politics of a particular pig festival (chap. 6) drew explicitly on versions of local historical discourse.

Lastly, Mosko regrets that *WGE* did not consider the symbolism of Mendi exchange more systematically. My forthcoming book treats in detail the ways in which specific forms of wealth (particularly, indigenous objects like pigs and pearlshells and introduced valuables like money) embody styles of sociality and constructions of time and space. The approach is perhaps not strictly "symbolic" (nor does it isolate "religion" as a domain of study). However, insofar as it concerns the contested meanings invested in objects specifically in the context of the historical engagement of cultures, I hope that it explores some of the territory the reviewers have identified.

NOTES

1. Concerning Mosko's reference to southern Highlanders as "loosely structured": I did not characterize them in those terms. On the page to which Mosko refers (p. 20), I cited Keesing's put-down of that notion: the point was that to describe a society as "loosely struc-

tured" constituted a failure of (specifically) structural analysis, which more dialectical approaches (such as Kelly's) aimed to overcome.

In this reply, "southern Highlanders" refers to peoples living largely in the Southern Highlands Province of PNG, including the high mountain valley grasslanders practicing intensive horticulture (like the Mendi and Huli) and southern "fringe" Highlanders and "mountain Papuans" (like the Etoro or Kaluli). I use the term "northern Highlanders" to refer to those central Highlands and northern "fringe" Highlands people (like the Enga, Melpa, and Chimbu) living in the Enga, Western, and Simbu provinces.

2. When I began my fieldwork in Mendi in 1977, relatively little had been published concerning southern Highlands societies, the work of Glasse, Kelly, and Schieffelin being notable exceptions.

3. Rumsey and Strathern also draw mutually contradictory inferences about the constraints bearing on exchange in Hagen and Mendi, suggesting interesting questions for further research.

4. Thus, we need to hold open the possibility of male control over network exchanges (as, perhaps, in the Wola case, although I agree with Strathern that Sillitoe's presentation is not entirely convincing) and of alternative modes of constituting differentiating and collectivizing relations in gendered terms (as in matrilineal cases outside the Highlands). For the Highlands, we need to bear in mind that women have been innovating collective forms of action in recent years (see, e.g., Sexton 1984). Concerning Strathern's point about Tom-bema Enga women in particular: As noted in *WGE*, the relationship between *tee* and *twem* needs to be explored as an example of exactly the sort of transformation to which I refer in the text above.

5. I am leaving aside discussion, relevant also to my comments concerning comparative strategies, about how we determine whether two anthropologists were in the "same place" (or, for that matter, in "different times," structurally speaking). It is obvious that where one draws the (geographical or temporal) line has everything to do with theoretical questions concerning the evaluation of differences and the construction of categories.

6. Ryan inserted the paragraph concerning the terms "tribe" and "clan cluster" into his contribution during copy-editing, after I had drafted this reply. The insertion occasions two comments. Firstly, while in *WGE* no weighty issues hang on the choice of one or another of these terms, for the record my use of "tribe" did indeed follow an "acceptable" definition (see, e.g., Sahlins's well known 1968 text, *Tribesmen*). Secondly, "tribe" captures Highlands social realities about as well (or as poorly) as most of the other conventional glosses used to refer to the largest territorially-based collectivities (all of which work less well than recently proposed terms, e.g., Strathern's "big names").

Finally, on a historical note: as I described in *WGE* and elsewhere, "clan clusters" (or whatever—that is, the largest *sem onda*) are no longer as rare as they may have been in Ryan's day, nor are they unnamed. Warfare or no, they appear to be expanding in the context of contemporary sociopolitical forces.

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